

THURSDAY, JANUARY 27, 1887

## SCIENTIFIC FEDERATION

IN an article on "Science and the Jubilee" a week or two ago, we referred to the possibility that the Royal Society might feel it desirable to consider whether it was feasible to signalise the present year of Jubilee by any new departure. It so happens that quite independently of the proposed celebration a very appropriate extension of the Society's usefulness to our colonies has been suggested and has already been accepted by one of the Australian colonies. This suggestion, and the action which the Royal Society has already taken upon the question submitted to it, really raises the whole question of the desirability of a scientific confederation of all English-speaking peoples.

The suggestion to which we refer was made in Prof. Huxley's Anniversary Address to the Royal Society little over a year ago, from which we make the following extract :—

"Since this Society was founded, English-speaking communities have been planted, and are increasing and multiplying, in all quarters of the globe,—to use a naturalist's phrase, their geographical distribution is 'world-wide.' Wherever these communities have had time to develop, the instinct which led our forefathers to come together for the promotion of natural knowledge has worked in them and produced most notable results. The quantity and quality of the scientific work now being done in the United States moves us all to hearty admiration; the Dominion of Canada, and our colonies in South Africa, New Zealand, and Australia, show that they do not mean to be left behind in the race; and the scientific activity of our countrymen in India needs no comment.

"Whatever may be the practicability of political federation for more or fewer of the rapidly-growing English-speaking peoples of the globe, some sort of scientific federation should surely be possible. Nothing is baser than scientific Chauvinism, but still blood is thicker than water; and I have often ventured to dream that the Royal Society might associate itself in some special way with all English-speaking men of science, that it might recognise their work in other ways than by the rare opportunities at present offered by election to our foreign Fellowship, or by the award of those medals which are open to everybody; and without imposing upon them the responsibilities of the ordinary Fellowship, while they must needs be deprived of a large part of its privileges. How far this aspiration of mine may be reciprocated by our scientific brethren in the United States and in our colonies I do not know. I make it public, on my own responsibility, for your and their consideration."

It would appear that the matter was at once considered by the Council of the Royal Society, because the next year (1886) Prof. Stokes, the present President, referred to the subject in the following words :—

"In his Presidential Address last year, Prof. Huxley suggested the idea, I may say expressed the hope, that the Royal Society might associate itself in some special way with all English-speaking men of science; that it

might recognise their work in other ways than those afforded by the rare opportunities of election to our foreign membership, or the award of those medals which are open to persons of all nationalities alike. This suggestion has been taken up by one of our colonies. We have received a letter from the Royal Society of Victoria, referring to this passage in the address, and expressing a hope that, in some way, means might be found for establishing some kind of connection between our own oldest scientific Society and those of the colonies. The Council have appointed a Committee to take this letter into consideration, and try if they could devise some suitable plan for carrying out the object sought. The Committee endeavoured at first to frame a scheme which should not be confined to the colonies and dependencies of the British Empire, but should embrace all English-speaking communities. But, closely connected as we are with the United States by blood and language, they are of course, politically, a foreign nation, and this fact threw difficulties in the way of framing at once a more extended scheme, so that the Committee confined themselves to the colonies and dependencies of our own country, leaving the wider object for some future endeavour, should the country concerned seem to desire it. The scheme suggested was laid before the members of the present Council, but there was not an adequate opportunity of discussing it, and it will of course come before the next Council. Should they approve of some such measures as those recommended by the Committee, they will doubtless assure themselves, in some way or other, that those measures are in accordance with the wishes of the Fellows at large before they are incorporated into the statutes."

What the Council of the Society has already done in the matter is of course unknown to us, as it has not yet been made public; but it is unnecessary to point out the extreme fitness of some such action as this being taken this year, if it is to be taken at all.

Undoubtedly the scheme foreshadowed by Prof. Huxley, if carried out in a proper way, may lead to a great many advantages. It is not unimportant that all the scientific organisations of Greater Britain should be welded into a homogeneous whole, so that, if at any time a common action should be necessary on any subject, the work could be done promptly and with the least strain. If any scientific organisation in a colony were affiliated with the Royal Society at home, there can be no doubt that it would be in a stronger position; that its standard of scientific work would be raised; that other kindred institutions would be more likely to be formed, on which a similar status might at some future time be conferred also. Such an organisation, too, would have a *cachet* conferred upon it, so that colonists would consider it a greater honour to belong to it, and would have a greater inducement to work for it, and to aid in all its efforts.

We can imagine some possible criticisms of Prof. Huxley's suggestions. For instance, it may be asked, Why should not Scotch and English and Irish organisations be treated in the same way? We think there is a very good answer to this objection. Any member of any of the British Societies, by taking a little trouble, may obtain any of the privileges which the Royal Society might confer upon colonists. To a great many British

Societies the publications of the Royal Society are sent gratuitously; there is no difficulty in obtaining access either to the libraries or to the reading-rooms when the members are in London, for the reason that all necessary knowledge as to how these privileges are to be obtained is of course possessed by those at home, whereas the member of a colonial Society who finds himself in England is in a very different position. He may know nobody, he may not know even of the existence of the facilities afforded, and he may leave England without having been present at any meetings of the Society, and without the knowledge that almost anyone who chooses can attend them. We are glad then on these and on other grounds that the question has been raised, and we believe that great good may be accomplished by acting on Prof. Huxley's suggestion.

### SUPERNORMAL PSYCHOLOGY

*Phantasms of the Living.* By Edmund Gurney, Frederic W. H. Myers, and Frank Podmore. (London: Trübner and Co., 1886.)

UNDER the title "Phantasms of the Living," three of the leading members of the Society for Psychical Research have presented to the world at large, in two bulky volumes running to upwards of 1400 pages, the evidence they have collected in support of the hypothesis of telergy and telepathy, or the influence of one mind on another, near or at a distance, without the intervention of the ordinary channels of sense. The division of labour, for such we may truly term it, seems to have been as follows: Mr. F. W. H. Myers writes an introduction and a concluding chapter on "A Suggested Mode of Psychical Interaction"; Mr. Edmund Gurney is responsible for the compilation of the body of the work, the presentation and criticism of the evidence; while in the collection of evidence and examination of witnesses Mr. Podmore "has borne so large a share, that his name could not possibly have been omitted from the title-page."

It is a matter of peculiar difficulty to do justice, in the space that NATURE can place at my disposal, to a work of such portentous bulk, one written in such obvious good faith, one on which the authors have bestowed so much time, labour, and thought, and yet one presenting views which no one who has learnt to believe in the parallelism or identity of neuroses and psychoses can accept without abjuring his scientific and philosophic faith. I hold it to be the duty of a reviewer not merely to air his own opinions, but to give his readers a sketch of the contents of the volumes before him. But how can one sketch in two or three columns so vast a mass of evidence, the chief value of which is, we are told, its cumulative nature? And if the reviewer owes it to his readers to present some sort of outline of the picture his author presents, he none the less owes it to himself, his author, and his journal, to endeavour to estimate the value of the original thus roughly outlined. Difficult as the task is, it must be faced.

The evidential part of the work opens with a record of cases which form, it is held, an experimental basis for thought-transference. The following description is given

by the Rev. H. M. Creery of experiments with his own daughters:

"Each went out of the room in turn, while I and the others fixed on some object which the absent one was to name on returning to the room. We began by selecting the simplest objects in the room, then chose the names of towns, dates, cards out of a pack, &c. I have seen seventeen cards, chosen by myself, named right in succession, without a mistake."

In similar experiments the investigating committee acted as agents. This excluded, in their opinion, the possibility of trickery. Tabulating the results thus obtained, they submitted them to Mr. F. Y. Edgeworth, who applied to them the calculus of probabilities, obtaining "a row of *thirty-four nines* following a decimal point," or practical certainty in favour of their being due either to collusion or to thought-transference.

Details are given of experiments on the transference of tastes under conditions which, in the opinion of the authors, precluded the possibility of collusion or deception. The following are a few successive results:—

| Substances tasted                | Answers given                               |
|----------------------------------|---------------------------------------------|
| Vinegar ... ..                   | A sharp and nasty taste.                    |
| Mustard ... ..                   | Mustard.                                    |
| Sugar ... ..                     | I still taste the hot taste of the mustard. |
| Worcestershire sauce ... ..      | Worcestershire sauce.                       |
| Port wine (quality not stated !) | Between eau-de-cologne and beer.            |
| Bitter aloes ... ..              | Horrible and bitter.                        |

Instances of the localisation of pains are given. "The percipient being seated, blindfolded, and with her back to the rest of the party, all the other persons present inflicted on themselves the same pain in the same part of the body. Those who took part in the collective agency were three or more of the following: Mr. Malcolm Guthrie, Prof. Herdman, Dr. Hicks, Dr. Hyla Greves, Mr. R. C. Johnson, F.R.A.S., Mr. Birchall, Miss Redmond, and, on one occasion, another lady. The percipient throughout was Miss Relf. In ten out of twenty cases the percipient localised the pain with great precision; in seven, the localisation was nearly exact; in two, no local impression was perceived; and in one, the last, the answer was wholly wrong."

Facsimiles are given of pictures reproduced by thought-transference. In a continuous series of six—none of which can be said to have been complete failures—two were reproduced by the percipient with great fidelity; even the comparative failures are instructive from their partial success. The position of the agent, we are told, rendered it absolutely impossible that she should obtain a glimpse of the original.

Such is some of the experimental evidence for thought-transference. Readers of NATURE will understand why this section of the authors' work, giving results obtained under conditions within control, is noticed at greater length than can be devoted to other branches of the evidence.

The next chapter deals with cases transitional between experimental thought-transference—in which both agent and percipient are voluntarily taking part with a definite idea of certain results in view—and spontaneous telepathy, where neither has voluntarily or consciously formed an idea of any result whatever. These transitional cases are